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Interactive comment on “A moving point approach to model shallow ice sheets: a study case with radially-symmetrical ice sheets” by B. Bonan et al.

Anonymous Referee #2

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The work presents a novel approach to ice sheet modeling – essentially an Arbitrary Lagrange-Eulerian (ALE) scheme applied to ice sheets. Inherent in the formulation of the problem is a computational mesh which adapts to the changing thickness profile of the ice sheet in a smooth and natural way by preserving the relative mass fraction in each computational sub-interval. The work is presented carefully and clearly, and the figures are clear and well-captioned. I think it will be a fine addition to the body of ice sheet modeling literature and I recommend it for publication in The Cryosphere after some modifications to enhance the clarity of the manuscript. I do have concerns that this approach might be difficult to scale up to the study of full Continental ice sheets.

From my perspective, this work follows in a rich tradition of Arbitrary Lagrange-Eulerian (ALE) schemes, but there isn't much in the way of reference to that line of prior work

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beyond the references to Baines et al (in the authors' defense, most examples of ALE hail from science domains with little obvious connection to glaciology). It would be good if you made that linkage in the introduction. Also, there is likely at least some commonality between your approach and those in the literature which it would be useful to reference. (I'm not at all saying "this has been done before" – there is much that is novel and specific to ice sheets, but it would be good to make that connection.)

One drawback I see to this approach is that moving the volume fraction is a globally-dependent action, since it depends on the spatial distribution of mass over the entire ice sheet for each time interval. Imagine, for example, the case where an ice sheet is locally in equilibrium, but *downstream* there is a change in accumulation. You'd be moving your mesh locally, even though nothing local changed. In that sense, it turns a purely local operation (updating the thickness profile once the velocity has been computed) into one with a global dependence; I could see this causing problems when applying this approach to continental-scale ice sheets, particularly when running on large parallel machines.

Another general comment – you don't seem to really engage with your example results – you describe the problems and then point the reader to the tables and figures. It would be nice if you commented on the results in the text (in part to ensure that a reader reaches the same conclusions that you do). I'd suggest summarizing the results in the tables, etc and pointing out how the results support your conclusions.

Minor points:

1. Scattered throughout the intro: "Adaptative" -> "adaptive"
2. p3, line 4: There exists -> *exist*
3. p4242, line 17 – Re: relying on dh/dr not being small (zero or close-to-zero) near the ice margin. This will likely be problematic for marine ice sheets, particularly near calving fronts)

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4. eqn 8: I'm assuming that you used the fact that $h=0$ at the margin to derive eqn 8? If that's the case, it would be helpful to mention that detail for those trying to follow you.
5. p. 4245, line 4: You take advantage of the fact that the lower boundary is at a stationary ice divide. How would you approach the more-general case of a freely-moving left-hand boundary?
6. p. 4245, line 20: "with SIA singularities can appear" – I'd suggest rephrasing as, "singularities can appear with SIA..." or something similar.
7. eqn 16: The use of "g" here is confusing, since it also is used for gravity.
8. eqn 16: Have you tried any other expansions? I could see a higher order reconstruction (like a PPM sort of thing) being useful to increase solution accuracy and convergence. Why did you choose the one you did?
9. p. 4246, line 13: "Suppose" -> "Suppose"
10. p. 4249, line 3: How did you arrive at your choice of timestep (here and generally)? Do you have any idea about the stability properties of your scheme?
11. p. 4253, line 14: I think this approach would be problematic for grounding lines if you're trying to track them as a part of your scheme since they're not generally going to move in the same mass-fraction conserving way that your scheme is built around.
12. p4256, Eqn B5: This obviously implies $U>0$. Also, I suspect that you could improve the accuracy by using centered differences everywhere except at the margin, and use a one-sided stencil (likely still a three-point one for accuracy) at the margin itself.
13. p4257, line 2: I'd suggest "first order" rather than "order-1" – "order 1" can be read as "O(1)" (i.e. non-consistent) as opposed to $O(dx)$, which is what you're trying to say here.
14. p. 4257, line 8, and elsewhere: "trapezoididal" -> "trapezoidal" (or is that an English spelling?)

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15. p 4257, line 12: You mention that you constrain the timestep dt to "preserve the order in Eq (B1)." Do you mean a situation where steepening of the ice profile results in mesh points overtaking each other? It would be good if you could expand on that (and any other timestep constraints you observe in practice).

16. p. 4257, line 15: "order-2" – "second order" is more common.

17, p. 4258, line 2 and B7: As before, I'd suggest "first order" to "order-1". Also, this is actually a *downwind" difference here, isn't it?

18. Table 2 – do the negative exponents imply that you're presenting convergence in terms of the number of points? Presenting convergence in terms of mesh spacing (dx) is more standard (and would result in the positive exponents that people are more-used to) – so, you should make that clear in the text.

19. Table 2 – I'm somewhat concerned that your total volume is only converging at a rate of $O(1.4)$ or so. Since total volume for the problems you've chosen (no flux across the margins) should only depend on the initial condition and the integral of the surface fluxes in space-time. It seems to be that you should be doing better for that one, and I'm concerned that the low accuracy there is polluting things elsewhere.

Interactive comment on The Cryosphere Discuss., 9, 4237, 2015.

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